

APR 4 1968

April 2, 1968

Dr. Joshua Lederberg
Department of Genetics
Stanford University School of Medicine
Palo Alto, Calif. 94304

Dear Dr. Lederberg:

Thanks very much for your letter of March 6. I have delayed answering you until I could digest the interesting material which you so kindly sent to me. It's a delight to find a scientist not only who writes well but also who thinks it proper to discuss such things as experimental design and the political-social implications of research before a non-scientific audience.

However, please reconsider your decision to remain "off the record." My book deals partially with the manner in which scientific developments are interpreted to the non-scientific public by scientists, politicians and science writers for both the specialized and mass media. As a Nobel laureate with a science column for public consumption, you are unique among these science interpreters. If I can develop enough material, I would very much like to devote a separate chapter to your philosophy and experience involving such interpretation. I do not think I can carry that off by attributing the information to "an unnamed Nobel Prize winner." Consequently, may I propose the following:

1. That I call you long distance on April 10 or thereabouts to discuss some of the points and questions raised in this letter.
2. To insure accuracy, I would tape record this conversation, make my own transcript and forward it to you for editing or further amplification. I would hope that after we reach agreement on the quality of the transcript as an accurate reflection of your views, I would be free to interpret them. I feel that a writer who accepts anything less than a free hand at such interpretation is little more than a hack. But if you disagree sharply with my interpretation on grounds of logic, taste or fact, then I will simply drop the chapter. Naturally I would do everything possible to change your mind, but if we cannot reach agreement, I will respect your wishes.

Let me add that in addition to using this material in my book, I would like to offer it to certain magazines if it appears suited for them. Norman Podhoretz, editor of "Commentary," expressed his interest to me last December in seeing certain chapters of the book and considering them for publication.

I suppose that the questions I am really asking in my book are these:

Would human intellectual progress be best served if the non-scientific public's role were limited to providing applause for genuine scientific achievement (both basic and applied)?

Does the non-scientific public have any business at all in wanting not only to understand -- but to influence -- the methods, directions and ethics of science?

4-17-68 JL had extended phone conversation

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Is the so-called self-policing mechanism of basic science so efficient that outside (non-science) observation, and possibly pressure, cannot be used to insure that the highest and most imaginative standards of experimental design and theoretical interpretation are used by scientists (particularly by those scientists with generous support by public funds)? To insure that altogether human motivations by scientists do not lead them to the diversion of research funds to personal or political use? To insure that the whole body of basic research, in the form of journal papers, reports to agencies, or papers presented at professional meetings, is screened as thoroughly as possible for the possibility of beneficial social application?

Is there any purpose at all in reporting to the public not only the "breakthroughs" (not my word) but also the breakdowns (which could range from faulty experimental design to outright fraud or conflict of interest involving public funds)?

These are very broad questions, so broad perhaps that they are unclear or imply anti-science motives. Please assume that while not making them here, I do know the distinctions between basic science ("neutral," "ennobling to the culture," etc.) and applied science; and that I do know the difference between the Scopes Monkey Trial-type of science knocking, the Bulletin of the Atomic Scientist-type of doomsday social implications analysis, and "enlightened negative criticism" designed to help science better achieve its own high goals yet not at inordinate public financial or social expense.

Implied in all of the above is my hypothesis (which I have attempted to test through some 100 interviews with scientists and science writers in the past six months, through perusing the Don K. Price, de Solla Price-type of science criticism, and through my own personal experiences as a science writer for five years and a newspaper political reporter for eight) that the reporting and interpretation of science to the public is incomplete. What is done is good -- the translation of jargon and the occasional treatment of social implications -- but that the "enlightened negative" critical function has been neglected.

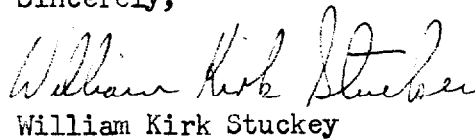
In gathering material for this book, I am using the techniques of a newspaper or magazine writer, and I am drawing heavily from a classical view (not always rigidly adhered to by the mass media) of the role of journalism in the United States. As originally conceived, this role is rather unique among nations. According to this view, the press is supposed not only to educate, entertain and record the principal events in the lives of people in a certain constituency-- but also to monitor the actions of power centers. Traditional power centers are government, industry, the military, and the like. The theory is that accurate information about the actions of power centers, good and faulty actions alike, will mobilize other power centers or the "public" to demand still more "good" actions and to demand the speedy elimination of actions contrary to the public interest. The press role, according to this ideal interpretation, is that of an ombudsman. And that is the role which I do not feel is being played by the press or other "power centers" regarding science. Whether or not non-scientist ombudsmen are needed is a point I am exploring and would like to discuss with you -- as well as whether or not science is such a "power center."

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I am enclosing selected excerpts of notes I have made for the book. They are intended to give you a more detailed view of my hypothesis. If you have already had too many details for one sitting, please ignore them.

My credentials for discussing such a field are minor but here they are nonetheless: Two awards for public affairs reporting by the American Political Science Association; an article on science writing in the September, 1966, issue of Harvard's "Nieman Reports"; and the writing of a chapter on scientific method for a Scott, Foresman Junior High science textbook.

Sincerely,

A handwritten signature in cursive script that reads "William Kirk Stuckey". The signature is written in dark ink and is positioned above the printed name.

William Kirk Stuckey

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